Interactive comment on "Effect of soil property uncertainties on permafrost thaw projections: a calibration-constrained analysis" *by* D. R. Harp et al.

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We thank the reviewer for a careful review of our manuscript and the insights she/he has provided. In the responses below, we describe how we utilized the reviewers comments to improve and clarify the manuscript. A PDF of the manuscript with marked-up revisions (additions in blue, deletions crossed out in red) is also provided and referred to below as the "attachment" with line numbers provided.

Minor comments:

1. Authors can add further references to introduction about recent model developments including surface vegetation insulation on soil thermal scheme: Chadburn et al. 2015;

Ekici et al. 2014; Wania et al. 2009

These references have been added in appropriate locations in the introduction (see line 57 of attachment).

2. It would be better to include a site information section for Barrow. It can explain the site conditions in particular climate, snow distribution and vegetation cover as well as soil characteristics for the observational location.

A paragraph has been added to the introduction to describe the arctic landscape of the Barrow Environmental Observatory (lines 91-100 of the attachment).

3. As I understand, the CESM outputs are used to drive the surface/subsurface model for calibration period (2013). Why not using the observed climate or at least showing the difference between observed and modeled atmospheric variables?

The observed climate was used for the calibration in Atchley et al., 2015. The CESM outputs are used to drive the projections for which no data are available. We have modified the abstract to clearly state that "measured" borehole temperatures were used (line 11 of attachment). We modified the first paragraph of the Methodology section adding a sentence to further discuss the measured data used in the calibration (line 115-117 of attachment). The captions of figures 3, 4, 15, 16, and 17 have been revised to clarify this point as well. We also identified that it is mentioned that the calibration data are measurements (observations) on lines 114, 117, 125-126, 143, 242, 246-247, and 521 of the attachment. There is a discussion of the proportion of the RMSE attributable to measurement imprecision on lines 252-254 of the attachment and an analysis of the proportion of measurements within the 95% confidence interval of the ensemble presented in figures 3 and 4 and discussed on lines 256-277 of the attachment. We therefore feel that this point is clearly stated. We thank the reviewer for bringing this to our attention.

4. What about the snow depth time series comparison? That would give important

information on changes and timing of saturation as well as other metrics.

We agree that snow depth time series comparison would be of interest and could provide important information. However, this seems out of context in the current paper which performs an uncertainty quantification of projections through the end of the century based on the calibration performed by Atchley et al., 2015. We suggest to prevent distraction from the main point of the paper (projection uncertainty due to soil properties) that we do not add this comparison, which would only be relevant for the time period of Atchley et al.'s calibration.

5. Why did you choose to calibrate for a single year of observational data? Wouldn't it be more useful to include as much observation as possible to constrain the parameters? Are there no available observations from other years?

Yes, calibrating for multiple years would be ideal. However the subsurface data needed to calibrate the model was not available prior to September of 2012, and the calibration was done during 2014 prior to that year's data becoming available The only complete year of data was for calendar year 2013. A sentence has been added to the Methodology section to explain this to readers (lines 115-116 of attachment). We thank the reviewer for pointing out that this was not clearly stated previously.

6. Further discussion about other arctic sites considering the different landscape types and consequent importance of potentially different parameters can be added.

A sentence has been added to do discuss the dependence of our results on the polygonal tundra of the BEO, and to acknowledge that there are other prevalent arctic landscape types (lines 99-100 of the attachment).

7. Please describe the term SI in Eq. 2, how do you calculate it?

We thank the reviewer for catching this omission. A description of this variable has been added (see line 322 of attachment).

8. In section 7 you mention "different climate scenarios" (p3369, l22). do you mean

different climate models? Since they all follow the same RCP8.5 scenario...

We agree with the reviewer that we should say "climate models" here. This change has been made (see line 434 of attachment).

9. I cannot see the pearson correlation coefficients on Figure 13

We thank the reviewer for catching this omission in our figure. It has been corrected.

Technical corrections:

We thank the reviewer for her/his technical corrections. They have all been implemented, and can be seen on the following pages of the attachment:

1. p3361 l5:"their are" should be "there are"

Fixed on line 236 of attachment

2. p3364 l10: "above freezing" should be "above freezing temperature"

Revised on line 313 of attachment

3. p3370 l6: "uncertianty" should be "uncertainty"

Fixed on line 444 of attachment

4. p3371 l13: "that" should be "than"

Fixed on line 475 of attachment

Please also note the supplement to this comment:
http://www.the-cryosphere-discuss.net/9/C1397/2015/tcd-9-C1397-2015-supplement.pdf

Interactive comment on The Cryosphere Discuss., 9, 3351, 2015.